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UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

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MEETING

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

(ACRS)

SUBCOMMITTEE ON THERMAL-HYDRAULIC PHENOMENA

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TUESDAY,

NOVEMBER 12, 2002

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ROCKVILLE, MARYLAND

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The Subcommittees meat the Nuclear Regulatory Commission, Two White Flint, North Room T2B3, 11545 Rockville Pike, Maryland, at 9:30 a.m., Dr. Graham Wallis, Chairman, presiding.

COMMITTEE MEMBERS:

GRAHAM B. WALLIS, Chairman

SANJOY BANERJEE, Consultant

THOMAS S. KRESS, Member

FREDERICK MOODY, Consultant

VICTOR H. RANSOM, Member

VIRGIL E. SCHROCK, Member

ACRS STAFF PRESENT:

PAUL BOEHNERT, Staff Engineer

ALSO PRESENT:

STEPHEN M. BAJOREK, NRC

LARRY HOCHREITER, Penn State University

RALPH ROSAL, Penn State, University

A-G-E-N-D-A

P-R-O-C-E-E-D-I-N-G-S

8:30 a.m.

CHAIRMAN WALLIS: The meeting will now come to order. This is a meeting of the ACRS subcommittee on thermal-hydraulic phenomena. I'm Graham Wallis, Chairman of the subcommittee.

The other ACRS members in attendance are Tom Kress and Victor Ransom. ACRS consultants in attendance are Sanjoy Banerjee, Fred Moody, and Virgil Schrock.

In today's meeting the subcommittee will discuss the status of the NRC Office of Nuclear Regulatory Research's rod bundle heat transfer program, underway at Pennsylvania State University.

Tomorrow, and the next day, we will continue review of the Framatome ANP-Richland S-RELAP5 realistic code version, and its application to PWR large-break LOCA analysis.

Portions of this meeting will be closed to the public for discussion of information considered proprietary in Framatome ANP-Richland, Incorporated.

Mr. Paul Boehnert is the cognizant ACRS staff engineer for this meeting.

The rules for participation in today's meeting have been announced as part of the notice of this meeting, previously published in the Federal Register, on October 23rd, 2002.

A transcript of this meeting is being kept, and the transcript will be made available, as stated in the Federal Register Notice. It is requested that speakers first identify themselves, and speak with sufficient clarity and volume, so that they can be readily heard.

We have received no written comments, no request for time to make oral statements from members of the public.

We will now proceed with the meeting, and I will call upon Dr. Steven Bajorek, from the NRC's Office of Nuclear Regulatory Research to begin.

DR. BAJOREK: Thank you very much. This is Steve Bajorek from the Office of Research. What we would like to do this afternoon is to continue on a series of meetings with this subcommittee that explains and gives the status of eight of our experimental programs.

In the past we've had the tests that are being run for phase separation at Oregon State. We've looked at the work by V. J. Dhir at UCLA for subcooled boiling model development.

Today we would like to give you a status and review of the RBHT program being conducted at Penn State University.

First, before I go any further, Jack Rosenthal wanted me to say that he apologizes for not being able to make the meeting today. He had a doctor's appointment that I guess the doctors would not let him out of. But he wanted me to let you know that he would truly rather have been here.

The RBHT program was started, I believe, in about 1998. Gene may correct me if it was earlier. The first two to three years of the program have been tied up, primarily, with construction, calibration of the bundle.

And at this time we are very pleased to be able to report that we've continued, or we've completed the bundle, or Penn State has, and they've run a series of reflood experiments, and now after a couple or three years, we are finally getting to the point where we have usable data.

And a group of us from the NRC has been up to Penn State, a couple of times, to inspect the facility, to witness some of the tests. And our initial reaction is we were very much impressed with what they've been able to do, the quality of the data we believe is quite high.

And it is hitting the objectives that were envisioned for this test program. If you take a look at the existing experimental data for reflood, be it from FLECHT SEASET, ACHILLES, there have been some shortcomings, either in that there weren't sufficient amount of instrumentation, or there weren't measurements that covered all of the various parameters that are believed to be important in the development of a truly mechanistic model for reflood heat transfer.

CHAIRMAN WALLIS: Could I ask you now, is the objective of this work is only to get data, or is it to develop models?

DR. BAJOREK: It is both. It is first to develop the data, and then to develop the models.

CHAIRMAN WALLIS: Because I think it would be very useful to predictions, as you do the experiments, so that you learn, you don't get a mountain of data, and then try to figure out what it means.

And then as you find you are learning things, you change the models, and then you maybe fine tune the data, or something. But it is dangerous just to take a lot of data without theory.

I don't see, yet, any predictions.

DR. BAJOREK: The Staff has not made any predictions of this. However, as part of understanding the facility, Penn State has used COBRA/TF to make predictions of the data, before they run the tests, and they've also followed up with their own model development, to try to predict the data that they were able to obtain.

This is -- one, it is very important, because you want to make sure that when you run the tests you don't impose conditions that are going to melt the rods, or do something that you don't want to happen to the facility.

DR. RANSOM: Wouldn't it be better to use TRAC-M for that purpose?

DR. BAJOREK: Yes, it would.

DR. RANSOM: And in fact, what I found in the past, almost invariably with the experiments that are made like this, that they create their own models, they aren't integrated with the main objective, which is to get it into the main systems code.

And so this creates a disparity later on, that the modelers, more or less, are accused of tuning the codes to try to get agreement when, in reality, the heat transfer correlation, or coefficient, has been derived from some model, you know, which the experimenter used.

So it would be nice if these two dovetailed.

DR. BAJOREK: No, I agree. I think that it would have been a lot better if we had TRAC-M ready to go, and were able to use it to make it the predictions.

Now, using COBRA/TF, however, we don't think is tremendously far off at this point. In TRAC-M right now is a reflood model that was developed in the late '80s, early '90.

And Joe Kelly, who has looked at this in a lot more detail than any of us, has concluded that this model just needs to be ripped out of the code, and we need to go back to something else.

The first cut of this is going to be what are calling and interim reflood model. And it is going to look a lot like COBRA/TF. We are going to try to take it back to that, and then start to replace those models with improved ones that we can get from the RBHT.

DR. RANSOM: One of the disadvantages of that approach is sort of like, you know, the subcooled V. J. Dhir's work, you create a model that doesn't fit in the structure of what you are trying to put it in.

So somebody in the end is going to have to make compromises, you know, to dovetail these together. And I would guess the same thing is true with TRAC-M, and -- I don't mean TRAC-M, but the COBRA/TF, that that is probably driven a lot by the familiarity of the principal investigator with that code.

But that doesn't help get it into, say, TRAC-M, or get new models into TRAC-M.

MR. SCHROCK: I think it would be helpful if there was available a brief assessment of what it is about the past work that has been found inadequate, and how those inadequacies motivate and define new experimental requirements.

I don't think we have ever heard that, clearly, about this program.

DR. BAJOREK: Actually I would have to go back and look, but I believe that when this program was started in '97, '98, that foundation was laid out. But I would have to go back and check that.

Now, one thing that --

MR. SCHROCK: Well, I don't think that is getting at the intent of my comment. I think we are about to go through discussion of details of instrumentation on a new set of experiences that would, essentially, retilling ground that was very heavily cultivated over a period of 15 years, in the past.

And I think we need to be reminding ourselves, as we go through this, what are the clear objectives that we need to keep focused on, and not just begin again and say, well, rod bundle transfer is important in large-break LOCA analysis, and we have to do it right, and we don't think we did it well enough before.

I don't know why we don't think we did it well enough before. I'm not arguing that it was done well enough before. But what I'm looking for is clear explanations of how we know now that it wasn't done adequately before, and what we think we can do to make it adequate in a new set of experiences.

I think you have to keep that sort of as a point of focus in these discussions.

DR. BAJOREK: Well, would it help, maybe this -- I think one of the problems that I think we've encountered, as we start to talk about what is in the code, and what we get from the test programs, is it starts to get too much for one meeting.

Would it be a decent idea to take meeting, in the future, describe what is in TRAC-M at this point, and how we are going to take these data and chaNge the models in what order?

MR. BOEHNERT: Well, we have a meeting scheduled in December to discuss TRAC-M, maybe you want to work that into the agenda.

DR. BAJOREK: We can work some of that in there.

MR. BOEHNERT: Yes.

CHAIRMAN WALLIS: Joe Kelly gave us a presentation, I would say, a couple of years ago, where he pointed out some of the anomalies in the present code, which needed to be fixed. I remember that.

But it wasn't quite clear to me how this tied in with this program, and what was going to be measured this time, which wasn't measured last time, with flood tests, which would resolve his difficulties.

So I think it would be useful if we could do that next month. Is Joe, who is the guy who is coordinating this with the model development?

DR. BAJOREK: Well, Joe is the guy who is in charge of the code development, and I work with Joe, looking at the models that are going into the code, but also taking a look at the experimental programs.

CHAIRMAN WALLIS: So you are the bridge between the theory and the experience?

DR. BAJOREK: Yes.

CHAIRMAN WALLIS: So maybe you are the guy who needs to come back in December.

DR. BAJOREK: Well, I will be here, anyway.

CHAIRMAN WALLIS: I will ask again why it is you again.

(Laughter.)

DR. BAJOREK: Maybe we will get more of our management there, or get an answer for it. But, no, granted that we do need to lay that out, and we have not done a real good job, at this point, at showing how we are going to take these data, and integrate these into the code.

But let us take that as an action, and start working that in at the next meeting.

DR. SANJOY: One other thing, just to continue Virgil's point. With the subcooled boiling work you made a clear case to us about what data was missing, and why that program had to go forward.

What I guess is still not clear to me, at least, I don't know to others, is what is the case for these experiments at all? I mean, at some point this case was made way back in history, the anecdotal evidence, that people at CSAU thought the tests were needed, therefore we did it, or whatever.

But I think we still need to make that case, once again, and continue to make that case. What is missing, why are we doing it, what are we going to find, how is it going to improve the models.

And that doesn't come through, from reading the material.

DR. BAJOREK: Okay.

CHAIRMAN WALLIS: Then we should ask, are we finding it, as we begin to look at the results.

DR. RANSOM: Right.

DR. BAJOREK: Part of the answer as to, I think, what has been missing, the earlier data, you see a little bit of it. This wasn't the intent of the overhead here.

But we have, overall, four major series of tests which are planned. Larry is going to talk with you, later this afternoon, describe the bundle, and talk about the transient forced reflood tests that were run since about last May.

Penn State has managed to run on the order of 32 experiments under varying conditions to cover a range of reflood rates, pressures, and subcooling. This kind of gives us our base cases for the reflood model.

The next couple of series of tests are going to start to go into questions on the reflood model that we don't believe have been adequately answered in previous test series.

Jumping here to the third one, the steam cooling, the droplet injection tests. One of the question marks that we've run into is what is the convective enhancement that occurs when you have droplets within the steam flow.

Earlier tests have been run, I believe, in a two by two bundle at UCLA, using glass beads, show that you get much better heat transfer when you have this dispersed phase in there.

But we really haven't been able to sort that out of earlier tests like FLECHT or FLECHT SEASET, to try to get at that individual mechanism, Penn State is going to be running a series of tests, one with steam only, but also with a rake of droplet injectors in the bottom of the facility.

So we are going to be able to get experimental data that gives us a known droplet content for a given steam flow. So we will be able to get that individual mechanism.

Larry is going to describe the instrumentation for this bundle, and that goes in here, here, the first, third and fourth. Some questions on what are the details that go on at the quench front, what is the progression of the void fraction in that vicinity, as it changes with subcooling and reflood rate.

Well, from earlier tests like FLECHT, the DP cells were, I think, a foot apart. Other facilities like G2, which is commonly used, I think they were two feet apart. It doesn't give us anywhere near the detail to try to determine what was the flow like right where quench was occurring.

The other thing that was very difficult to get out of earlier experiments, was some of the droplet information. When we were trying to use the FLECHT SEASET data in development for the models for best estimate at Westinghouse, trying to determine what was the reflood droplet size, what was that initial size, and how did it chaNge as it went through grid spacers.

It was very difficult, because in the FLECHT SEASET experiences you had measurements of, I think, 3, 6 and 9 feet, but very few droplets. The 3 foot may have half a dozen droplets, the 9 foot, for a couple of tests, may have only on the order of 50 to 100.

And they weren't broken down for all of the tests, there were only some very select ones. Now, a lot of that had to do with the instrumentation at the time, which really meant taking some good high speed movies, and get somebody with a good set of eyes, projecting it on a screen, and going frame by frame, to look at how the droplet changed.

It took forever and a day to try to get information for one test. With newer instrumentation we are able to get that much quicker, you can get it at multiple locations, and we are going to be able to get better models for how does the droplet originate, how does it change as it goes through an individual grid, and how quickly does it evaporate away in a steam of a certain temperature.

All of that information was there, to an extent, in some of these earlier experiments, but it was so sparse it made it very difficult to get models that you were confident in, and get them quantified, to a degree of accuracy that you could apply them, then, to a PWR, or a BWR experiment.

So I think where you will see some of

those questions answered is what you are going to get that is going to improve the models, and where those uncertainties are. It is in those processes that are right now buried in the reflood models, that we can't get at, unless we get some of the better experimental information.

So we've got four series to try to segregate that out, using the newer instrumentation. The second one, which I haven't really mentioned on here, those add almost a more basic question, as how do the flow patterns develop and transition within a rod bundle.

CHAIRMAN WALLIS: How do they measure interfacial drag?

DR. BAJOREK: We don't measure it directly. I guess I think of it more in terms of using the increased number of DP cells to get at the change in void fraction, as opposed to a direct measurement, then using carryover measurements of the steam flow, and liquid flow, coming out of there to deduce what should be the right interfacial drag.

DR. KRESS: Is that between the steam and the broad bundles?

DR. BAJOREK: Steam and the droplets or the films, which were there.

DR. SANJOY: What was done at WINFRED?

DR. BAJOREK: Those were the ACHILLES tests. I'm trying to remember, or recall exactly what was there in those tests. I don't believe they had much in the way of steam probe measurements in those tests.

I think it was more of a traditional rod bundle. I want to say it was on the order of 50 or 60 rods. The rods were instrumented, there was relatively sparse steam probe measurement, no droplet. Larry, do you remember what that is?

DR. HOCHREITER: Larry Hochreiter, Penn State. The WINFRED tests were, basically, a set of reflood experiments. It had, I think, a 69 rod bundle. They did have delta P cells on it, but I don't remember them ever reducing that to get any void fraction data.

And they primarily looked at temperatures, and the heat transfer, itself. To my knowledge there was no droplet data, there were no steam probes in that facility, that I'm aware of.

So it is really their first shot at running a reflood test. And I think what it was used for was basically to confirm the types of heat transfer that they would have been predicting for a

Sizewell type plant.

I think it was really designed to give them a basis for looking at other tests, and other models.

DR. SANJOY: Why didn't they use the DP cells to get the void fraction?

DR. HOCHREITER: I don't know.

DR. BAJOREK: Larry, I think they did get void fractions out of the DP cells, just for a few of the tests.

DR. HOCHREITER: Okay, I just never saw it.

DR. SANJOY: Are the databases available to us?

DR. BAJOREK: Yes, we have some of them, it is hard to find. We do have a report, and some of the experimental data. But, again, I forget some of the details of the bundle, but it wasn't a complete set of data.

You get the heat transfer coefficients and the void fractions, but you don't have droplet sizes, you don't have carryover fractions, and if you don't have the steam temperature measurements, you really don't have that consistent set of information.

DR. SANJOY: Do you remember the pressure range?

DR. HOCHREITER: I think it went up to 60 PSI four powers.

DR. BAJOREK: Yes, it is low pressure, there is a lot of low pressure data with it.

DR. HOCHREITER: They also use it for level swell, they also use it to look at the effect of nitrogen injection. In fact that became the International Standard Problem number 25, I think.

DR. KRESS: The effect of the droplets, as best as I remember, was pretty sensitive to the size distribution for a given amount of liquid in there.

Will we be able to get size distributions out of the --

DR. BAJOREK: Yes, yes.

DR. KRESS: Even inside of a bundle?

DR. BAJOREK: Yes.

DR. HOCHREITER: Well, I will explain that.

DR. KRESS: Line of sight.

DR. HOCHREITER: That is right.

DR. BAJOREK: But line of sight, but you get droplet sizes, and also total carryover fractions, which I think are really very important to have in these tests.

I was going to save this more for the end, but since a couple of the questions have kind of come up along this, where does it really fit in with our plans for the model development.

As I mentioned, some of the reflood tests are complete at this point, and over the next couple of years they will be moving into the interface, what we re calling the interfacial drag tests, and these droplet injection tests, over the next couple of years.

Right now our plate is fairly full when it comes to our ability to take all of the data that we have from our experimental programs. Because of the need for advance plans, our work right now is trying to take the ATLATS data, and develop models for phase separation that we would use in TRAC-M.

We did take your suggestion to heart back in June or July, about trying to integrate some of the subcooled boiling models in earlier. Originally we weren't going to be able to get to that, but due to some clever accounting we were able to start that work a little bit earlier.

And we have a student at UCLA who is taking their models, put them into a stand-alone package, which I've asked at this point, so that we can integrate this into the code.

The mechanistic model development --

MR. SCHROCK: What happened to the interim reflood model?

DR. BAJOREK: That is ongoing right now. That is -- the interim reflood model is where we are taking out the existing package in TRAC-M, and essentially replacing it with the package that had been there, or very close to the one in TRAC-PF1, which is about as close to COBRA/TF as you can get, the way the numerics are right now in TRAC-M.

MR. SCHROCK: See, the trouble I have, Steve, is that I'm convinced that when you do detailed experimentation that is related to mechanistic model development, that you have to have some idea of what you mean by mechanistic reflood models, in order to establish what is required of the experiences, what is to be measured, where, how accurately, and so forth.

I don't see how you know what those things are from the description that you've given here. So do you learn that from old models that you've had in the code, codes, that you've twitched, and done different things with, to gain some insight?

Or what do you do to get all of that down? And how can you convey that to us?

DR. BAJOREK: I think the right way to do that is to step through some of the existing models in the code, show where we think there are shortcomings. And, more importantly, point where we think they can be improved, within the numerics of the code.

In answer to your question, have you done that, I haven't done that with TRAC-M. But in working with code like COBRA/TF, you can find that changing models for interfacial heat transfer interplay with steam temperature, which plays upon the droplets size, which then impacts your heat transfer at the top of the rod.

MR. SCHROCK: But what does models mean, here, in this context; is that correlations, or is it first principle analysis of the process, or what?

DR. BAJOREK: I would say it is, primarily, correlations. It is those models and correlations for the various processes involved in reflood heat transfer. Interfacial heat transfer, the droplet breakout, heat transfer coefficients from the rod, as a function of the regime, and also the droplet content, transition boiling near the quench front.

And I think entrainment, that is another one that is very difficult to pin down.

MR. SCHROCK: So it is models meaning

correlations in a format compatible with the structure of TRAC and RELAP type codes?

DR. BAJOREK: Almost. Because one reason this has deliberately been delayed is in order to install a third field into TRAC-M. Right now we are dealing with the code numerics that does not allow us to model, simultaneously, droplets and liquid films.

And we want to start that work early next year, so that we are able to have more flexibility in developing those models.

CHAIRMAN WALLIS: Well, does liquid films, are liquid films measured in the Penn State experiment?

DR. BAJOREK: No.

CHAIRMAN WALLIS: But if you need to somehow coordinate the experiment with the model --

DR. BAJOREK: Not -- well --

CHAIRMAN WALLIS: You need to measure the things that are in your model.

DR. BAJOREK: But I need to have the droplet field so I can break it up as I go through grids.

CHAIRMAN WALLIS: So there is also a liquid on the wall, maybe there isn't a liquid on the wall in that --

DR. BAJOREK: It depends on the regime. The low temperature regimes, yes. But --

DR. RANSOM: Steve, I think one thing you just mentioned, at least in my experience has been the root of the problem, is the transition boiling regime. Never been able to explain the precursory cooling that takes place.

And I'm talking about a macroscopic effect, because these nodes tend to be on the order of half a foot to a foot. So you've got to explain the average heat transfer behavior over that kind of region of the fuel, in order to explain the progress, say, of a quench front, either boiling down, or heating up.

I think boil down is easier, but the reflood part has always been harder. So I guess what we ought to look for is how are you going to shed light on that transition boiling regime in the vicinity of the quench front.

And while I'm talking, I guess, I would be surprised if even the principal investigator wouldn't prefer a separate effects experiment, where he could get more detail on what is going on, right in the region of that quench front, rather than, say, rod bundle time.

DR. BAJOREK: In fact, as part of the Thermal-Hydraulic Institute we are proposing to do a test very much like that.

DR. RANSOM: In this facility, or?

DR. BAJOREK: Not in this facility, but to use a smaller, separate effects facility where you can focus on some of the details, and use what you learn there in conjunction with the rod bundle, to come up with better models.

DR. RANSOM: That has a better chance of finding the answer, I would think.

DR. BAJOREK: I mean, in a way we are looking at some of the details of the quench front in much the way that the program was structured at UCLA for subcooled boiling, where he had small scale experiments to take a look at how the bubbles form, and developed, versus subcooling and flow on a flat plate.

Very easy geometry, easy to photograph, easy to measure, and then use a small rod bundle to verify things. So we are thinking in terms of that.

DR. SANJOY: There's been an enormous amount of work done at that scale in tubes and simple geometries. So we don't want to, we want to make sure that this is not just repeated in some sense.

Because even the inverted annular regime there has been modeling at Berkley, certainly, there has been extensive set of experiments. I would like to know, exactly, before we launch into this, what it is that we will learn, compared to what it is that we already know.

Because I think that the modeling efforts have really not taken into account a lot of these old experiments, where very detailed measurements were made. I can probably give you my thesis on that.

MR. SCHROCK: So my question may be, is the past inadequacy of code predictions for this portion of transients a consequence of the structure of the code, or inherent lack of experimental basis for the fundamental processes?

If you've not taken the data from past experiments to look at the phenomena processes that are involved there, sufficiently, you may not have used them adequately to know whether you need new experiments, or whether you can gleam that information from the old ones.

So it is unclear to me, still, how the motivation occurred originally, and what the vision is for a new set of experiments that are going to fill in the inadequacy of the past work.

DR. BAJOREK: Let me take that as an action.

CHAIRMAN WALLIS: Maybe you can think about it. I was just going to propose that we hear from Larry Hochreiter, and then you come back. You were asked to come back at the end of the day, anyway, and you can tell us what you've learned.

I mean, they've done these 32 tests in reflood, what did they learn which enlightened you, from those tests?

DR. BAJOREK: Well, I will let Larry show the movie, and hopefully --

CHAIRMAN WALLIS: Well, that is very qualitative, isn't it?

DR. BAJOREK: Well, that part of it.

CHAIRMAN WALLIS: Well, I would like to see, actually, since they must be far enough into the program, where you could say, you know, this was the state of the art before they did the tests, and this is what we've learned so far, and this is an advance in something.

DR. BAJOREK: Well, I will let Larry show the movie. But I think one of the very eye opening things is what are first order effects in these experiments, versus what may not be as, you know, as important.

I think we will see the grid effects --

CHAIRMAN WALLIS: Ideally we ought to have some measure of uncertainty before, and uncertainty after, and how you've reduced the uncertainty by getting more information.

DR. MOODY: A minute ago the subject came up separate effects test, and I was looking at the abstract of this. Maybe I missed something, the report describes, so on, and so on, to conduct a systematic separate effects test.

Well, that is what has been done here, is being done, right?

DR. BAJOREK: Right.

DR. MOODY: These are separate effects?

DR. BAJOREK: Yes.

DR. RANSOM: Distinguished from an entire system, but still it is a rod bundle test, which -- and you are looking at things, I think, that are occurring locally.

So, yes, it is separate effects, and it is a single bundle.

DR. MOODY: Would it be system boundaries?

DR. BAJOREK: Large separate effects test, and small separate effects test.

CHAIRMAN WALLIS: Would it be time to go on to the Penn State presentation and then you can come back later?

DR. BAJOREK: Yes.

CHAIRMAN WALLIS: And perhaps give us a bit more wisdom on what you've learned from it all.

DR. SANJOY: Before you go, Steve, just a question. You said three fields. And if I recall, there was three fields in track way back, at some point.

DR. BAJOREK: There was one version where they did have three fields. I'm not sure whatever became of that.

DR. SANJOY: I mean, I think Tony Hurt put it in -- and Kenneth Sly. Oh, Ken Williams, okay. What happened to that?

DR. HOCHREITER: It got published as a thesis.

DR. SANJOY: It was never put in?

DR. BAJOREK: COBRA/TF has just the two fields, but part of our vision is to get that third field in there, to make it behave a lot more like COBRA/TF.

DR. HOCHREITER: Larry Hochreiter, from Penn State.

The first thing I learned is don't go second or third at an ACRS meeting. What I wanted to do was to show you some of the results that we've gotten to date, in the program.

The comments that, I think, Dr. Schrock made, and Dr. Wallis made, about the program, the genesis, the origin, the goals, and this type of thing, we did present this to the committee, but it has been a couple of years.

MR. BOEHNERT: Yes, you did make a presentation.

DR. HOCHREITER: And I think there was, I know there was at least one, maybe two presentations that Joe Kelly and I did to the Committee, when we were designing the experiment, and basically providing the rationale for why we were going to do these types of test, and what new information we were going to get, what information was lacking, and what information this facility, these tests would provide, that would fill that gap.

So as I go through my presentation I will try to point out those areas, okay?

This is a joint NRC Penn State program that is being performed at Penn State. The contract was initiated in November of '97. Again, at Penn

State it is a program between the College of Engineering, and the Applied Research Laboratory.

The principal investigators are myself, Dr. Bill Cheung, and Dr. Thomas Lin. Dr. Lin works at the Applied Research Laboratory.

Again, the reason for doing this through Penn State, at the Applied Research Laboratory, is they have a very good infrastructure for performing experiments. They do, primarily, work for the Navy, and this type of stuff. So they have a very good experimental infrastructure.

Now, in terms of background, and of course you have seen all this, what we are primarily concerned about is a loss of coolant accident, and primarily the reflood portion of the loss of coolant accident.

And the driving force for it was the improvement in the Best Estimate models. When CSAU came about, and was used, the types of powers that were being examined from the best estimate point of view, were actually fairly low.

In the CSAU study I think the peak kilowatts were something around 9. Right now plants are being licensed with the best estimate methodology with the peak kilowatts per foot someplace around 15.

So what has occurred, in the interim, is that the margin that was identified, from the best estimate analysis has basically been consumed by the utility to basically broaden the operating envelope for the plants.

And so you are now seeing best estimate peak cladding temperatures that are in the same range of the appendix K calculations that we were looking at, perhaps, five years ago.

So now the emphasis on the accuracy of the best estimate method becomes much more of a critical item, because you now have a reduced amount of margin because you have consumed the margin in the analysis.

Again, the reflood is usually the period of interest, because this is where the peak cladding temperature occurs. The heat transfer rates are the lowest. I think, as Dr. Ransom indicated, predicting the precursory cooling is the key item here, because this is where the peak cladding temperature is occurring.

And you have several different heat transfer mechanisms. And I will show a figure on that. The area that we are looking at, and trying to concentrate, primarily in this program, is a highly dispersed non-equilibrium flow, where we have superheated steam with entrained liquid droplets, which are at the saturation temperature.

The quench front is progressing up the rods, but this takes time. In the meantime the cladding temperatures can continue to heat if you don't predict the heat transfer rates accurately.

And so your peak cladding temperatures, even for your best estimate models occur during reflood in nearly all these situations.

This is just a schematic of what we are talking about, for a flow regime, where we have basically a quench front moving up, and typically the cases we are looking at you have low injection flow, or flooding rate, so there can be boiling below the quench front.

The heat release from the rods generates high steam velocities which basically shear and entrain the liquid, it gets carried up in the rod bundle.

CHAIRMAN WALLIS: That blue stuff is liquid?

DR. HOCHREITER: Yes.

CHAIRMAN WALLIS: I don't see the film boiling.

DR. HOCHREITER: Well, I tried to stay

within the lines when I colored it, and it is probably buried behind these tons of liquid here.

But you are right, in this case, where low flooding rate, there is a very, very short area of inverted annular film boiling. In facility, you could argue that it is really not even inverted annular film boiling, because it depends on the void fraction that is occurring in here.

But the point of interest is actually further up in the rod bundle, where you are basically being cooled by steam, with drops. And it is the interaction between the steam and the drops that is providing cooling.

DR. RANSOM: Are your experiments exploring the different reflood rates?

DR. HOCHREITER: Yes.

DR. RANSOM: Are your experiments simulating different reflood rates all the way from the low to the --

DR. HOCHREITER: Yes. I will show you a table of conditions.

DR. KRESS: Are they also simulating, right here, an initial temperature of the rods?

DR. HOCHREITER: Yes, but we have -- there is, obviously, a range of initial temperatures.

DR. KRESS: Yes.

DR. HOCHREITER: We have chosen to keep the initial temperatures lower than what you might find in calculations. The calculated temperatures at the beginning of reflood can be as high as 1,600 degrees fahrenheit.

We have been started our tests at 14. We have also -- can I defer that until I show you the table?

DR. KRESS: Sure.

DR. HOCHREITER: The dispersed flow of film boiling region is the region that we are trying to focus on to get better quality data. This is one region, the quench front is the other region.

And there are several different heat transfer mechanisms that can occur in this region, and looking at the different models in the computer codes, the codes try to predict all of this in one area or another.

The problem is that some of the models will overpredict a particular phenomena, other models will underpredict the phenomena. And so if you get the right answer you are never really too sure of why you got the right answer, other than you might have been lucky that day, okay?

So as Steve had indicated, what we were trying to do in these experiments, we will run the reflood heat transfer experiments, but then we will also do steam cooling, and drop an injection experiments.

We are really trying to decompose the disperse flow of film boiling, period, experimentally, and look at these different effects as best as we can.

CHAIRMAN WALLIS: What is disperse flow film boiling?

DR. HOCHREITER: It is a continuous steam phase which is superheated with dispersed liquid droplets, which are at the situation --

CHAIRMAN WALLIS: Why is it film boiling?

DR. HOCHREITER: It is film because you have vapor against the wall.

CHAIRMAN WALLIS: The droplets don't hit the wall?

DR. HOCHREITER: The droplets don't hit the wall.

DR. KRESS: That is the important regime, because that is what you have when you get close, most of the way up to the peak clad temperature.

DR. HOCHREITER: Right.

DR. KRESS: Now, it seems to me like one

could make some real good analytical estimates of each one of these.

DR. HOCHREITER: That is right.

DR. KRESS: And, you know, the problem I had with it is how many droplets do I have in there, and what is their size.

DR. HOCHREITER: Exactly.

DR. KRESS: And I made some calculations at one time, this is like 15 or 20 years ago, and I seem to remember that what governed was just two little things, the heat transfer between the vapor and the wall, and the heat transfer between the liquids and the vapors.

And I forgot, the radiation just didn't enter into it very much.

DR. HOCHREITER: It is small.

DR. KRESS: And so if I could, again, handle on those two things, and then basically it is boil down to what is the droplet size and distribution, and how much is in there, because you could almost use existing correlations for that heat transfer between the droplets and the vapor.

And almost existing correlations between the vapor and the wall.

DR. HOCHREITER: Well, I think it is a

little more complicated than that, because of things like this.

DR. KRESS: Well, yes, what happened was, that was -- you are right. The crux of it was that droplet size, and size distribution, and the amount in there changed every time you passed the grid.

DR. HOCHREITER: That is right.

DR. KRESS: And you never knew how to deal with that.

DR. HOCHREITER: The test at Oakridge clearly showed that --

DR. KRESS: Yes, and that is what I was looking at, the Oakridge test.

MR. SCHROCK: But you say that the codes try to solve this problem, but then you point out that the grid spacer is a complication. The calculation in the code has axial nodes that are probably too large to deal with the detail that you are talking about here.

So it is unclear what one means when one says that the code tries to address this level of the physics, it is not possible in an axial node that has a lot of variation from end of it to the other, to deal at this kind of level.

So this is what I mean by identifying

where is the difficulty, is it the structure of the code, is it the quality and extent of the experimental information?

I'm convinced that it is the latter. I think that it is, somehow, the code and the existing data developed somewhat independently, and so they don't mesh well, and it is hard to use the existing data in the framework of existing codes.

So one can change the structure of the code, one can find other experiments that might fit the structure of the code better than the existing ones. But I think you have to define what your objective is, what are you going to do in the end.

I don't think you can have a successful resolution of this by getting more detailed experimental data that are beyond the capability of the code to properly utilize those data.

DR. HOCHREITER: Yes, but I think you could use the experimental data to tell you what the code should do, and what level of detail you might have to put into the code if you want to represent the phenomena correctly.

MR. SCHROCK: I think you can judge that from the data and the code that you already have.

DR. HOCHREITER: It depends on the data,

okay? If there were no spacer grids in these, or the spacer grid effect was very, very small, you probably could survive with larger nodes.

But what I'm going to show you today is that you are probably going to have to go to finer nodes. Because the spacers you have not is --

DR. RANSOM: Well, most of the codes do have a fine mesh rezoning in the conductors, at least.

DR. HOCHREITER: But that is at the quench front, primarily, following it.

DR. RANSOM: Right. And it seems like the main mechanism that is missing is the bottom one that you have on the slide. And I think I just heard you say that one doesn't occur.

DR. HOCHREITER: No, I didn't say that.

DR. RANSOM: That the liquid can't touch the wall.

DR. HOCHREITER: In the area where the PCT is occurring the liquid does not touch the wall. AS the temperature drops to the point where you can have contact, that obviously does occur.

DR. RANSOM: Right, but it seemed to me there was some mechanism in which there is enhanced heat transfer near the quench front, that must be tied up with liquid --

DR. HOCHREITER: Well, I don't --

DR. RANSOM: -- contacting the wall.

DR. HOCHREITER: -- disagree with you at all, at all. In fact, one of the things that we tried to do in the program is we have faster data sampling rates so we can get the quench more accurately, when the rods do quench, and we have fine zones of delta P cells so we can get an estimate of the void fraction, when the rods are quenching.

DR. KRESS: I think that is the important parameter, you need to know how much liquid gets into the system, and that is the importance of that quench front.

DR. HOCHREITER: But the quench front, quench front is like a boundary condition, all right? Because it provides the basis for the entrainment, which is swept to the upper elevations.

The PCT that you are concerned about is at the upper elevations. So you need to know the history of the generation of the entrainment. In fact, in discussions with Steve, and Joe Kelly, and other people, and even when we did this at Westinghouse, the largest uncertainty in our calculations was the entrainment.

Not only the amount of entrainment, and

then what it looks like in the flow. And what we've tried to do in these experiments is to capture that information as accurately as we can.

We put collection systems onto the facility to give us a rapid indication of when we get entrainment, how much entrainment we get, and then we have very fine delta P cells across the bundle to indicate what the mass storage is, in the facility, as a function of time.

And for most of the tests we converge and get about a five percent uncertainty in the mass and balance. For a test that lasts 1,000 seconds, which is pretty good, I think.

But this is a phenomena that is more prevalent at the quench front, whereas just these phenomena are more prevalent further up into the bundle.

And, as I said, if you did not have spacer grids, you probably could get away with coarser noding. But when you put in something like this, the changes dramatically, I think it is dramatic, anyways.

The flow behavior, the dispersed flow behavior, then I think you will have to go to finer axial nodes as, I think, you were suggesting. We did a bunch of noding sensitivity calculations with

COBRA/TF, at Penn State, when we were trying to predict these types of tests.

Because we would do pretest predictions for every test, as the cost of the rod bundle is outrageous. And we certainly don't want to burn out any rods. The program that Steve shows there goes out for another three or four years, and there is no provisions to rebuild a rod bundle.

And this rod bundle costs a half a million dollars. So we do not want to burn up any rods. So we would do tests and calculations until the cows came home. And this is part of the reason why we set a lower initial temperature.

But there are other reasons that make these tests different, and I think, give you better information than what exists today.

This is the test facility, basically. And I have to --

DR. KRESS: Theron, he needs his mobile microphone.

DR. HOCHREITER: If I don't move around I fall asleep.

MR. SCHROCK: This last picture doesn't look much --

DR. HOCHREITER: I'm sorry?

MR. SCHROCK: This las picture doesn't look much like your low reflood rate cartoon on the previous one.

DR. HOCHREITER: Well, this is a bunch of pipes and tanks.

MR. SCHROCK: I'm not on this one yet. Your mechanistic diagram, and your low reflood rate.

DR. HOCHREITER: This is a blowup of --

CHAIRMAN WALLIS: Larry, don't touch the screen.

MR. BOEHNERT: Don't mark on the screen, only Tom can do that.

(Laughter.)

DR. HOCHREITER: This picture would be occurring up in here.

MR. SCHROCK: And your focus is mainly on that region, in your experiments?

DR. HOCHREITER: Well, we have provided instrumentation to focus on this region, but we've also provided more detailed instrumentation to focus on this region. We tried to cover the transient.

Not only low flooding rates, but high flooding rates.

MR. SCHROCK: Well, I'm recalling some earlier experiments which showed, as this picture suggests, a sort of tongue of liquid moving up and breaking off, having enough momentum to rise some distance beyond the point where it is broken off, but it doesn't have enough force left acting on it to carry it on up, so it falls back.

And so you have liquid being thrown ahead and falling back, thrown ahead and falling back. It has always seemed to me that that is, inevitably, important in getting at entrainment rates.

Is that going to be studied in these tests?

DR. HOCHREITER: Actually if you -- this region can be between eight inches and a foot above quench front. It is the low void fraction region, lower void fraction region.

And what we did, in the experiment, and you will see a picture of this, is that we have pressure cells every three inches. So as the quench front, and it is over about three feet, if I remember correctly.

So as the quench front enters that region we will get a finer definition of the local void fraction. We also set the rod instrumentation up such that within each void fraction cell range we would put thermacouples in the rods that would be approximately in the center of the region of where you would be measuring the void fraction.

Because the cell is going to measure the average void fraction, once you correct it for pressure drop, and so forth.

MR. SCHROCK: So over some period of time, as well as space?

DR. HOCHREITER: That is correct.

MR. SCHROCK: And time is large compared to the periods of oscillation that I've described, I think?

DR. HOCHREITER: Yes, because these experiments, I might as well say this now, one of the unique things we did in these experiments was we kept the power constant. In nearly all the other reflood experiments they simulated a K power.

That makes it more prototypical. Our objective was not to be as prototypical as those previous experiments, but rather to provide us data, better quality data, that we could use for model development and assessment.

And by keeping the power constant you basically stretch out, particularly, the dispersed flow film boiling period, you stretch the entire experiment out, for that matter.

So you get, basically, quasi-steady state disperse flow film boiling. Now, it is not perfectly steady state because the quench front is slowly advancing up into the bundle.

But you get a longer period of quasi-steady state where you can make measurements of vapor temperature, drop sizes, rod temperatures, and with the delta P cells in for a void fraction.

In addition to the mass that is carried out of the facility, measure the steam flow that is carried out, we measure the liquid flow that is carried out.

DR. SANJOY: But you have a power profile, don't you?

DR. HOCHREITER: We have an axial power profile, but we kept it simple.

DR. SANJOY: But it was sort of peaked, if I remember?

DR. HOCHREITER: Right, at about the ten foot elevation.

DR. SANJOY: So, in fact, you've got something prototypical about that?

DR. HOCHREITER: Yes.

DR. SANJOY: If you had kept it uniform, that would have made more sense to --

DR. HOCHREITER: No, we debated that, and it just -- we looked at a bunch of profiles that were being used for best estimate analysis, and the worst answers you get, in the best estimate code, are for profiles where the peak is above the mid plain.

And it is simple logic, because you are just further from the quench front.

DR. SANJOY: But from the viewpoint of what you are saying right now, which is to get data which is for model building, you know, and keep the quasi-steady approach, and so on, that won't give you a quasi-steady approach.

DR. HOCHREITER: Well, we do get a quasi-steady approach.

DR. SANJOY: Because the power is going up, right?

DR. HOCHREITER: The local in your power is going up, but the temperature response are almost steady with time. I will show you some of the temperatures.

DR. SANJOY: Well, let me get back to the void fraction measurements. You said you corrected for pressure drop, and so on? How do you do that?

DR. HOCHREITER: We do a mass energy, we are doing this now, we are doing the calculations now.

We know the exit flow rates, we measure those. We know the vapor temperatures in the bundle, so we know the degree of non-equilibrium.

We know the rod heat flux distribution from the thermacouples in the heater rods. We can back calculate down into the bundle the local quality, real quality. So we can calculate the local steam flow, the local liquid flow.

DR. SANJOY: Equilibrium quality?

DR. HOCHREITER: Non-equilibrium, because we are using a measured vapor temperature. Based on that we can estimate a frictional pressure drop for the cells, and correct the cells.

Now, the correction will be the most inaccurate for the highest void fractions. The correction will be more accurate for lower void fractions, but the effect of the correction for lower void fractions is less important, because the elevation then is more dominant.

DR. SANJOY: How much is the correction?

DR. HOCHREITER: We haven't gotten to that point yet. We are just getting to that point now. But in previous, I've done this before, in other tests, but in a much, much coarser scale, and it was approximately a 10 percent effect.

DR. SANJOY: And you have accelerational pressure drops, too?

DR. HOCHREITER: Yes, that is accounted for. All three pressure drop components.

CHAIRMAN WALLIS: Now, in the region where globes of liquid are going up and falling down again, they follow F=MA, and gravity acts on them, but it doesn't create any pressure drop, the acceleration and deceleration of the masses of liquid is completely balanced, or mostly balanced by gravity.

So the usual kind of decomposition into gravitational and frictional doesn't work.

DR. HOCHREITER: Let me think about that.

CHAIRMAN WALLIS: If you just juggle it, tossing balls in the air, they go round, and round, and round, there is no pressure drop from the juggling the balls.

DR. HOCHREITER: I understand what you are saying, and I went through that argument. And somehow I convinced myself that the cell would measure this. Now, maybe I better go back and --

CHAIRMAN WALLIS: -- acceleration terms for the --

DR. HOCHREITER: Well, there is an acceleration term.

CHAIRMAN WALLIS: But it is not the average acceleration, they go up and they come down again.

DR. HOCHREITER: If it is a -- no, local effects like that we are, obviously, not going to get. Because, first of all, the cell is going to measure the average across the bundle.

And we have to do, like Dr. Schrock says, you will have to look at that in time, in addition to space.

CHAIRMAN WALLIS: You don't have independent void fraction by means of gammas, do you have some sort of a --

DR. HOCHREITER: We talked about that in the program, and because of funding constraints, that was never --

DR. SANJOY: The idea could be, at least, checked against people who are using gamma densitometers and bundles and see how accurate it is.

DR. HOCHREITER: There was a report on that, I think, in the FIST program.

DR. SANJOY: Well, they are using it in Costine, the densitometers. Franz Manger has done some work, so you could probably check it out, at least, to see whether it is accurate or not.

We always felt it wasn't. We made a lot of gamma densitometer measurements with tubes around the reflood point, one of my students did this way back in the '80s. And we never felt comfortable with pressure drops.

But maybe you've worked out how to do it, I don't know.

DR. HOCHREITER: Well, it depends upon the sensitivity of the cell. These are actually very sensitive cells.

DR. SANJOY: Right, very small pressure differences.

MR. SCHROCK: In your report you describe some commercial instrumentation which has outstanding accuracy.

DR. HOCHREITER: For the cells?

MR. SCHROCK: No, no, for a number of different kinds of instrumentation that I couldn't tell you, off the top of my head, without looking back at the report, which one I'm thinking of.

But you give a single figure for the accuracy of that instrumentation, which is a manufacturer's claim.

DR. HOCHREITER: That is right.

MR. SCHROCK: Do you have any independent corroboration of the manufacturer's claim, and do you have a reason to believe that the uncertainty is not a function of the scale?

DR. HOCHREITER: Okay, that is four questions.

MR. SCHROCK: That is okay, that is all related.

DR. HOCHREITER: Actually we've discussed this with the NRC. To me what you get from the manufacturer would be the minimum error.

MR. SCHROCK: I saw that that is the way you are referring to it, I don't understand why it is minimum, but --

DR. HOCHREITER: I'm sorry maybe it is a maximum error, maximum error. And we just went through this for the data report where this is Ralph Rosal in the back, and he looked at the trace of the signal from the instrument through the electronics, through the DAS system, and so forth, and you get a most probable error.

And that is based on the manufacturing information. So that is absolutely the absolute best it could ever, ever be. And that is an error, okay? The uncertainty due to the flow of conditions, the pressure, the pressure variation, these are usually much larger.

And we have to get that information by looking at the experiment, and it is larger. So when you design the experiment you try to design the instrumentation to minimize Any errors or uncertainty in the instrumentation.

But there is an additional component, if you really want the answer, that you have to add on to it, which reflects the uncertainty in the experience.

MR. SCHROCK: Well, I guess my comment and question was motivated by a couple of things. One is an inherent distrust of manufacturer's claims for the accuracy of instruments that are black boxes. Buy my instrument, plug it in, and get this accuracy of measurement. It is not a sound engineering approach.

Secondly --

DR. HOCHREITER: Wait a minute, let me address that. To address that we calibrate.

MR. SCHROCK: Well, that is why I asked if you have an independent corroboration of that level of accuracy.

The other point is that in almost every case, when one looks at the accuracy of the instrument, the accuracy of a given reading, that accuracy will depend upon whether it is at full scale, near full scale, half scale, a tenth scale, or whatever.

And when one reads the report one has the impression that you've not considered the issue of the accuracy of your experimental measurement in terms of where in the full scale you are operating.

DR. HOCHREITER: That is probably true.

MR. SCHROCK: We can come back to it -- I mean, it is true you have not?

DR. HOCHREITER: We considered the range that it has to cover, but I think when we did the uncertainty assessment for estimate we did not consider, as far as I remember, I don't think we considered -- I don't think we considered where we were in the range, I'm not sure, it has been so long since we wrote that.

CHAIRMAN WALLIS: Do we need to move on to your pipes and tanks?

DR. HOCHREITER: Pipes and tanks.

DR. RANSOM: May I just suggest one thing? You know, as far as this void fraction question, measuring with hydrostatic pressures, it can be answered with your code, because it does include all the forces that are involved.

DR. HOCHREITER: That is correct.

DR. RANSOM: And --

DR. SANJOY: Not for falling back.

DR. RANSOM: Pardon?

DR. SANJOY: Not really, because what you see in these experiments, I don't know if you ever --

DR. RANSOM: Well, I'm not saying everything, but the hydrostatic pressure is affected by the transfer of the body force on the liquid, to the vapor, through the interfacial drag, that is the mechanism that actually changes the hydrostatic pressure along the tube.

DR. SANJOY: But the flow is oscillating, remember, in this. And it is not linear with the velocity difference. So when you have a non-linearity like that, it doesn't balance, exactly what Graham was saying.

DR. RANSOM: Well, my main point was that it won't answer the question on your experiment. But if you go look at a code, you know what the void fraction is, and you know what the void fraction is that you would calculate from the hydrostatic pressure change.

CHAIRMAN WALLIS: As long as the average is representative of what is happening.

DR. HOCHREITER: Exactly, it would have to be the average, and I think it would be better to look at the actual pressure drop, rather than the void fraction, which is inferred.

DR. RANSOM: Well, you have two things. I mean, you take the pressure drop, you calculate, convert it to a void fraction, and you can look at the void fraction, which is --

DR. SANJOY: You are trying to balance the code against the experience, directly.

DR. RANSOM: No, I mainly want to do an experiment with the code and say, okay, how does the real void fraction compare with what I would calculate from, say, a hydrostatic pressure change in the vapor field, and how big is that difference. That can be done.

Without knowing anything about the experience, it just tells you what kind of errors you might expect.

CHAIRMAN WALLIS: Sometimes the liquid is running down the wall, and you actually have a negative friction in your theory, which is --

DR. RANSOM: That doesn't affect -- that affects the hydrostatic pressure, also, because it tends to resist the, you know, the vapor flow.

DR. SANJOY: Well, the code is based on a model.

DR. RANSOM: Yes.

DR. SANJOY: The model actually, when you average the equations, the way they are done, only holds if the oscillations are not large compared to the mean flow.

You can show, in fact, that the model breaks down because friction is non-linear, because of the square. So these mean field models that are used don't use for oscillatory flows very well. I mean, this is pretty well known.

DR. RANSOM: Because of virtual mass effects, and things like that.

DR. SANJOY: Well, not even that, it just comes through friction, I mean, directly. It is a non-linear term, right?

CHAIRMAN WALLIS: Well, I guess we are not going to discuss the model at all, today. I think we should move on to the experiment.

DR. SANJOY: That is why I'm saying the model may not be -- it would be nice if you took a densitometer and do it.

CHAIRMAN WALLIS: He is not making a presentation on the model, so I guess we have to ask him about his experiment.

DR. HOCHREITER: This is the test section, here, these are the delta P cells that are hung off the test section. And, again, there is a fine, fine group of cells in this region here, to capture the quench front effects, particularly in attempt to correlate heat transfer with void fraction.

The objective was to get a set of data, better data than exists right now, because most of the experiments right now, these things are at least a foot to two feet apart. So the objective is to get a better set of data where you can correlate the as-measured heat transfer, versus void fraction.

So you can come up with a relationship between heat transfer and void fraction, particularly in the region above the quench front.

DR. MOODY: Where on that background is your peak?

DR. HOCHREITER: Right about here.

CHAIRMAN WALLIS: But the only unusual feature of this system is the pressure oscillation dampening time.

DR. HOCHREITER: Yes.

CHAIRMAN WALLIS: It seems to me that in the real reactor you have a compliance of the system, it is not clear to me that the pressure oscillation damping tank is prototypical of that compliance, whether or not you get oscillations in reflood is related to the whole system.

DR. HOCHREITER: Well, we weren't trying to be prototypical here. What we wanted to be able to do is control the pressure more accurately, particularly in here.

CHAIRMAN WALLIS: Yes.

DR. HOCHREITER: Because pressure variations, and we found this out because -- I can't find where the valve is. This valve could cycle, would cycle, actually. And you would drive pressure oscillations in here, this result in invalidating a large number of tests, because it was like an imposed boundary condition on the facility.

DR. SANJOY: I think your point is well taken, but in a real system, in a prototypical system you could get oscillations, as Graham pointed out, especially in some of these new concepts where the reflood is gravity driven.

DR. HOCHREITER: All the reflood in every plant is gravity driven.

DR. SANJOY: Right, so then you will get the --

DR. HOCHREITER: Yes, but it is the

oscillations that come from the downcomer, because that is where the head is, that is what is driving the flow into the reactor.

DR. SANJOY: Right, but in a sense these oscillations depend on the details of the system, and you could get oscillations, right? And they can affect entrainment.

DR. HOCHREITER: Oh, they will.

DR. SANJOY: Yes.

DR. HOCHREITER: I mean, we can simulate that effect in this test.

CHAIRMAN WALLIS: Maybe you need to do tests with different amounts of oscillations.

DR. HOCHREITER: There have been tests that have been run like that. For instance, we ran some in the FLECHT AND FLECHT SEASET program. We didn't really have a lot of different oscillations.

We also ran gravity reflood tests at both those programs. So, I mean, there is some data out there, and you do get these surges that go into the bundle. And then you are dependent upon the driving head, and the resistance downstream.

CHAIRMAN WALLIS: I think the surges tend to help your quenching?

DR. HOCHREITER: They do. But then the

flow drops out. The flow reverses and drops out, and you are basically heating up adiabatically.

CHAIRMAN WALLIS: We don't know what happens in a reactor.

DR. HOCHREITER: No. I do remember the Long Sung Tong, that reactors don't oscillate, I do remember that.

Anyways, in our test facility we have an upper plenum here, which react as a first stage space separator, and we have liquid collection tanks. And the idea was to quickly measure the liquid as soon as it got up here.

So before separation we measure the liquid in a small tank first, then a larger tank.

CHAIRMAN WALLIS: -- from your write-up, how would the upper plenum work. You have something about a weir, and trying to make sure there was no back flow from the upper plenum.

DR. HOCHREITER: Yes.

CHAIRMAN WALLIS: But I couldn't see, there was no detail in the --

DR. HOCHREITER: Well, not on this figure, no.

CHAIRMAN WALLIS: -- so that geometry, even in your big fat report I couldn't see any detail of what happened up there.

DR. HOCHREITER: This housing extends into the upper plenum. So the liquid that gets separated out here does not run back down. And then you drain it very quickly, and you vent -- these tanks are vented to the plenum.

This is a standard steam separator with a liquid collection tank, so any liquid that gets carried out by the steam is separated and measured here. This says, we talked about the suppression damping tank.

So the liquid measurements, for the liquid of the bundle are here, here, and here. This is the steam flow, and then these pipes are heated, and these tanks are heated.

All this system here is heated saturation.

CHAIRMAN WALLIS: How do you know how to slice that damping tank?

DR. HOCHREITER: We looked, actually, at the ACHILLES program, and looked at the volume in ACHILLES versus the volume in the tank, and scaled it based on that. Because the ACHILLES had very, very good pressure control.

CHAIRMAN WALLIS: The purpose is to damp out oscillation not to --

DR. HOCHREITER: Really, the purpose is to prevent any oscillations that come from trying to control the pressure to feedback under the system. We had this problem in FLECHT SEASET, and we were trying to cure that problem.

And ACHILLES did not have that problem, and the ACHILLES people came to Westinghouse and picked our brains for several days. In fact Ralph Rosal went over to England to go through a design review on ACHILLES.

And the thing that the British added, which was very different, was this tank. And they wound up with better pressure control than we had in FLECHT SEASET.

We also have provisions for a boiler which can provide the single phase steam into the facility. We have an injection port within the housing to be able to inject water droplets of different sizes. We've actually run tests on injection nozzles, and measured the droplet sizes.

So we can run experiments now. Those tests where you would have steam coming in here, and you would inject water, would be more of a steady state, or much more of a steady state from boiling test.

Because what you are doing is you are getting rid of the quench front, okay? So in theory you could run those tests for much, much longer period of time, separate out the data.

And there is other instrumentation, within the facility, where you can measure more details within the rod bundle themselves. We have traversing steam probes, which I will show you a schematic of, and then pass one around.

And when you run a steady state test, like a steam cooling test, you can traverse these steam probes, and there is 13 of them.

CHAIRMAN WALLIS: Can you tell us what they actually measure?

DR. HOCHREITER: Temperature.

CHAIRMAN WALLIS: They measure their own temperature, but how is it related to what is going on around them?

DR. HOCHREITER: Well, I'm going to show you some of that.

CHAIRMAN WALLIS: -- they quench, so they go down to the saturation temperature.

DR. HOCHREITER: And then come back up.

CHAIRMAN WALLIS: Come back up. Were they measuring radiation from the rods?

DR. HOCHREITER: There is a radiation component that has to be factored in.

DR. KRESS: Now, you can run those tests at a higher temperature, because you don't have to worry about the burn-up.

DR. HOCHREITER: Well, you always have to worry about burning up these rods.

DR. KRESS: yes, but you don't have to worry about going into the departure from nucleate boiling time.

DR. HOCHREITER: No, because you are already there.

DR. KRESS: Yes. But you could run them at higher temperatures, I think.

DR. HOCHREITER: I will tell you what limits some of the temperatures, are going to be this apparatus up in here.

DR. KRESS: You've got limitations on the steam temperature coming in there?

DR. HOCHREITER: Right.

DR. KRESS: Okay.

DR. HOCHREITER: I mean, I think we went to metallic seals, up here, for that very reason.

MR. SCHROCK: These droplets that are sprayed in, are sprayed into the rod bundle, how do you prevent them from impinging directly on the wall?

DR. HOCHREITER: We've done some experiments where we've positioned these, and these holes are electromechanically machined, so they are very precise holes. And we inject, into the subchannel center.

CHAIRMAN WALLIS: Pointing downstream?

DR. HOCHREITER: Well, pointing in the direction of the steam flow, okay? And so we run some bench type experiments on that. Will they impact the walls? I'm not sure.

MR. SCHROCK: But it is not directed towards the wall, it is not like a --

DR. HOCHREITER: No, no --

MR. SCHROCK: -- spray head, they are opposite a lot of different directions, it is one little jet that --

DR. HOCHREITER: It is like -- what, three small holes per subchannel, if I remember correctly.

MR. SCHROCK: There is three small jet streams axially down each --

DR. HOCHREITER: Right, subchannel. And we can, obviously, bury that. We can reduce the number of holes so you reduce the amount of liquid flow.

And these are inserted tubes that slide in between the rods, and with the holes pointing upward. And we have not run any of those tests yet. Those are tests to be run in the future.

This is a cross section of the bundle. It is a seven by seven. These are, basically, rods which are hollow tubes, these are not heated. So there is 45 heater rods, okay?

And when we look at the data we primarily look at the inner 5 by 5. We do have instrumentation, of course, all the way around it, and also on the housing.

The rods are made out of inconnel, the housing is made out of inconnel. The thermocouple sheaths inside the heater rods are made out of inconnel.

And the reason for this is to try to prevent differential thermal expansion which can lead to bowing, either of the housing, or a bowing of the rods. Inconnel is a better high temperature material to be used, anyways.

Inside the bundle we have eight of these spacer grids.

CHAIRMAN WALLIS: Now, is there some liquid that goes to the outer wall, and is very different from the liquid in the middle?

DR. HOCHREITER: Well, we try to minimize this access flow area to prevent that. We also heat the housing by radiation from the rods. So when you start the tests, you start the tests full of steam, and then you basically pulse the bundle to heat the housing.

And we typically get the housing temperature up to around 900 to 1,000 degrees fahrenheit at the peak power location.

And I'm going to show you some housing quench fronts, and some rod quench fronts.

DR. MOODY: Is this little flag on each one of these the skin flow?

DR. HOCHREITER: Yes, that is a simulation of a prototypical mixing vein grid.

Westinghouse was kind enough to send us drawings without dimensions, which that was fine. And then we took those drawings, and we made manufacturing drawings, and we had a company make the grids for us.

The supports we have to use are different than what are used in prototypical grids. Plus we have to leave more clearances, okay? When the bundle is cold the rods should rattle, all right?

In other words, we don't really use the

grids for anything more than spacing of the rods. In the reactor the grids are really used to support the rods.

And the reason for this is we don't want the rods to get bound up in the grids when it is hot. And we don't want the rods to bow.

MR. SCHROCK: How do we cope with the problem that the manufacturer's spacers may, probably, be different than the ones that you examine in these experiments?

How dependent will your "models" be on the specifics of these spacer geometries?

DR. HOCHREITER: Well, what we are planning on doing is we will characterize this grid primarily in terms of a blockage area. Now, we may have to go to a finer level than that, particularly when we are looking at the veins.

We have to look at the fraction of the flow that is swept by the veins. But the calculations that I did years and years ago basically say that, you know, the steam can flow around things, the drops go straight through.

So I think to the first order of magnitude, the thing that is important is the amount of blockage area, because that is what is going to shatter the drops.

MR. SCHROCK: So if there is no clear line of sight through a grid spacer, the detail of the geometry no longer matters, the liquid is going to impact the wall?

DR. HOCHREITER: That is right, that is what I think.

MR. SCHROCK: All right.

DR. HOCHREITER: This is the test facility. These, again, are all the delta P cells. One of the unique things we did in this facility, which caused us much agony and grief, was to use very large windows.

CHAIRMAN WALLIS: Why are those delta P cells so enormous?

DR. HOCHREITER: I have no idea. But I will say that we got a good deal on this.

DR. SANJOY: Are they Pizio?

DR. HOCHREITER: No, strain gauge. I'm looking at Ralph. I think it is strain gauge, the delta P cells, they are strain gauge, aren't they?

DR. ROSAL: No.

DR. HOCHREITER: What are they?

DR. ROSAL: It is a diaphragm.

DR. HOCHREITER: All right.

DR. SANJOY: Because it is very sensitive, right?

DR. ROSAL: -- the gap between the sensor and the diaphragm, and they are very strong.

MR. BOEHNERT: How do they measure that gap?

DR. ROSAL: Your question is how do you measure the delta peak?

DR. SANJOY: So sensitively, yes.

DR. ROSAL: The sensor is a very large diaphragm, and both sides have a, they measure, I guess, the gap, the movement of the diaphragm.

DR. SANJOY: Is that a capacitance, or optical --

DR. ROSAL: It is like a capacitance detector, and it is very sensitive, it is very strong. You can overload one side, 5,000 PSI, and the diaphragm doesn't disturb.

DR. SANJOY: Are you also -- this is not a flash mount, little pressure -- or how is the --

DR. ROSAL: The taps are on the housing.

DR. SANJOY: So it goes into the wall with a little tap?

DR. ROSAL: There is a cavity in the DP cell where the two lines come, the high side and the low side come in.

CHAIRMAN WALLIS: How do you know what is in the lines?

DR. HOCHREITER: You probably can't see these, these are all sloped downward.

DR. ROSAL: THE lines are tubes, 2/8ths in diameter, they come from the wall of the housing into the --

CHAIRMAN WALLIS: They have to be full of liquid to work properly?

DR. ROSAL: Yes, there is liquid.

DR. HOCHREITER: -- in the reference leg, too.

DR. ROSAL: It will maintain the reference leg full all the time.

DR. SANJOY: Do you purge liquid through them?

DR. ROSAL: Yes.

DR. SANJOY: Or how do you keep them full?

DR. ROSAL: Yes, you purge.

DR. SANJOY: Cold liquid?

DR. HOCHREITER: Yes. And the stand-off keeps them cold.

DR. ROSAL: They are away from the housing, so that the reference leg is at room temperature all the time.

DR. SANJOY: And does this liquid actually get into the test section?

DR. HOCHREITER: You mean the liquid in here?

DR. SANJOY: Yes, what happens to the liquid in the lines?

DR. HOCHREITER: Well, you start off with these as full as you can get them, okay? But they are sloped towards the cell.

DR. ROSAL: There is a slope and the diameter is large enough so we don't capture gas bubbles in it.

DR. HOCHREITER: But what you are trying to do, you are trying to always make sure the reference leg on the cell stays filled.

DR. SANJOY: Well, each of the taps have to stay filled too, right?

DR. HOCHREITER: No.

DR. SANJOY: They don't?

DR. HOCHREITER: No.

DR. SANJOY: Because they are horizontal?

DR. HOCHREITER: Yes. Slight slope.

DR. SANJOY: And the hole itself is it very carefully deburred, or --

DR. HOCHREITER: Yes.

DR. SANJOY: And you've checked it all?

DR. HOCHREITER: Yes.

DR. MOODY: What was the diameter, Larry, did you say?

DR. HOCHREITER: The tubing is 3/8ths of an inch.

CHAIRMAN WALLIS: How big is the hole?

DR. ROSAL: One-eighth of an inch. From experience we determined that for two phase flow --

CHAIRMAN WALLIS: So a bubble in the hole have --

DR. ROSAL: For a two phase flow you have to have a larger tap than for a single phase flow.

CHAIRMAN WALLIS: Yes, otherwise you can get a bubble in the hole, or a drop on the hole.

DR. ROSAL: And it stays there.

DR. HOCHREITER: As I said, one of the big things that is different in this facility is the size of these windows. These windows are almost a foot. And we positioned the windows to be able to view spacer grids.

We also heat the windows, just like we heat the housing. In fact, are clam-on radiant heaters, we use to heat the windows. And then we can photograph through here, we also use a digital camera, laser illuminated digital camera system.

CHAIRMAN WALLIS: You try to keep the windows dry, then?

DR. HOCHREITER: Yes, until the quench front on the rods basically approaches, and then it basically overwhelms.

CHAIRMAN WALLIS: So droplets that come up there evaporate when they hit the window?

DR. HOCHREITER: Well, they probably don't even hit the window because the window is so high.

DR. KRESS: Now, you take photographs of the windows?

DR. HOCHREITER: Yes, I'm going to show you some of the results of the data, and we will look at a film clip.

These are traversing steam probes, okay?

DR. KRESS: To get the steam temperature?

DR. HOCHREITER: To get the vapor temperature.

DR. MOODY: What window material do you use?

DR. HOCHREITER: Quartz.

DR. MOODY: Quartz. You are not etching your quartz yet?

DR. HOCHREITER: No, it cracks first.

DR. MOODY: We had an awful lot of quartz in our lab.

DR. SANJOY: We went to sapphire.

DR. HOCHREITER: We've had a lot of problems with the windows, because just a small distortion in the bundle, in the housing, and you have to use a high temperature seal.

These seals were like 800 dollars each. And then when you tighten down, because you have such a large window, any distortion that you are trying to compensate for, with the seal, you wind up cracking the edge of the windows.

So, again, we lost time because we were forever taking windows out, replacing windows, replacing seals, and so forth. And it really became a problem.

This is what these traversing steam probes look like. We have three 15 mil thermacouples which are, basically, held onto a piece of inconnel shim stock, and that is what is being routed around.

These can move in and out between the subchannels. For the majority of the tests these probes were positioned at the center of subchannels. But I'm going to show you data for a probe being at the center of a subchannel, and for a probe being at a gap between two rods.

These, because they are so small, and again these tests are sort of quasi-steady state, you will measure the vapor temperature, the non-equilibrium vapor temperature for a much, much longer time period, than we ever achieved in FLECHT, or FLECHT SEASET, okay?

The quench front can almost be within a foot or less before these things will totally completely wet. Notice totally, completely wet. You will get dips down to the saturation temperature.

And at least I have an interpretation of what you should use for the steam temperature.

DR. RANSOM: And what is time constant for those?

DR. HOCHREITER: 15 mil TCs, I don't know, I don't remember. Short.

CHAIRMAN WALLIS: So you correct for radiation, you calculate the radiation heat flux, and you have a correction for the --

DR. HOCHREITER: Yes. Now, we haven't done it in these thermacouples, these specific thermacouples. We did those types of calculations on other bare thermacouples we used, in previous experiments, and on the aspirating thermacouples we are using FLECHT SEASET.

And the temperature levels for the rods are actually relatively low here. So the radiation effects, I think, are going to be small. Because they were small, at much higher temperature levels, for previous experiments.

DR. RANSOM: There must be an error in your report. You say here it is .813 millimeters, those are much smaller than that, I believe.

DR. SANJOY: He says .15 inches.

DR. RANSOM: Well, it is written here .813.

DR. HOCHREITER: Well, we probably screwed up.

DR. SANJOY: Because there are two or three different thermacouples.

DR. HOCHREITER: Oh, that is correct, I'm sorry. Vic, there are different thermacouples. There is another set of thermacouples, and I don't have a figure for those.

DR. RANSOM: It says the vapor or steam temperature will be measured using miniature thermacouples having a diameter of 1.813 millimeters.

DR. HOCHREITER: I think those refer to

the thermacouples which we attached to the spacer grid.

DR. RANSOM: Yes, it says they are attached to the spacers, and to the traversing steam probe rates all having a diameter of .381.

DR. HOCHREITER: We put additional vapor temperature measurements attached to these spacers, and had them point down, brought the instrumentation out across the spacer, over to the pins that were in the corner of the bundle, down those pins, and out the bundle.

We also had temperatures which were, thermacouples which were brazed into the metal of the spacer. And those were routed across the spacer, back over to the pins that were on the outside of the bundle, and brought out of the bundle.

So we can measure the spacer temperature, the vapor temperature. Of course we had rod temperatures measurements. I don't have a figure to show this, but what we did, when we set up the heater rod instrumentation, and maybe I should go back to --

DR. RANSOM: The heater rods have thermacouples on the inside of the --

DR. HOCHREITER: Yes, on the inside of the cladding.

We know there is going to be a heat transfer enhancement downstream of the spacers, because the Oakridge data show that. Actually we didn't have any data of our own that showed that. But primarily looking at the Oakridge data I can't think if there is another set of data we looked at.

And I had developed a real simple exponential decay multiplier that you apply to a convective heat transfer coefficient, based on single phase data.

We use that prediction to basically pick the positions for the thermacouples downstream of the spacer, okay? And we would look at these inner five rods, and choose different rods, and symmetrical positions, where we could basically measure the detailed temperatures downstream of the spacers.

We also set up the traversing temperature, vapor temperature measurements to measure the temperature of the vapor, downstream of the spacers. And we would have two or three of these between spacer grids.

So we can get an idea of what the vapor temperature behavior was. Then we had vapor temperature probes sticking off the grid, pointing in the upstream direction to measure the vapor temperature coming into the spacer grid.

So we had a lot of instrumentation, detailed instrumentation, in and around the spacers, because the feeling was that the spacer grids have a first order effect on the disperse flow of film boiling, because they are going to change the drop sizes.

They are going to change the amount of mixing that is in the flow. Now, you can't figure that all out from one reflood test. So that is why in the program we were going to run the steam cooling test, only, and look at the convective heat transfer behavior, particularly with these spacers, then do droplet injection, and look at what happens to the steam cooling behavior when you inject droplets with these spacers.

DR. KRESS: Larry, when I was looking at the Oakridge data that effect was in there, that you said. And I couldn't decide, at first, whether this was an enhanced turbulence, entrance reeds in effect, or the effect of droplets getting broken up.

And I started using a Webber number criteria to get the droplet size.

DR. HOCHREITER: Right.

DR. KRESS: And what I had trouble was,

once I broke it up with the Webber number, they were always that size. And I didn't get any enhancement going through subsequent ones.

So I had to build into the model, an agglomeration model of some sort, to make the droplets get bigger again so I could re-break them up. And so I had trouble. I finally concluded this was more a breakup of the profile, velocity profile, into entrance reason effect, is what I finally concluded.

But I don't know of --

DR. HOCHREITER: Well, that is exactly why we want to do this experimentally, but separate these effects out. So we will run steam cooling tests only, and get that effect. Then we will introduce drops, and we will look at what the change is in the steam cooling.

Because I think there are other effects, in addition to drop breakup. Steve alluded to this earlier. The drops seem to do something irregardles of the grids, to enhance convection.

CHAIRMAN WALLIS: Don't you get bigger drops after the grid because you have a film on the grid, which gets re-entrained again?

DR. HOCHREITER: That is not what our measurements show.

CHAIRMAN WALLIS: You don't get that?

DR. HOCHREITER: No, it showed just the opposite.

CHAIRMAN WALLIS: You think the grid is dry, then?

DR. HOCHREITER: No, most of the tests the grids are wet, because we measure it.

CHAIRMAN WALLIS: Usually drops which come off a film are bigger than the ones in the flow.

DR. HOCHREITER: I know. So 1.9 times whatever.

DR. KRESS: The trouble with trying to invoke some agglomeration of droplets to make them bigger, is that there were too few of them in there, if I used any ordinary agglomeration type of -- they didn't see each other.

DR. HOCHREITER: It is a very sparse population.

CHAIRMAN WALLIS: But, Tom, your model was based on your imagination?

DR. KRESS: Yes, I was looking at the Oakridge data and trying to imagine what was going on. That is exactly right.

CHAIRMAN WALLIS: He is going to have reality checks.

MR. SCHROCK: Larry, I'd like to come back with my earlier question about the grid spacer and whether your grid spacer is going to provide information that will be applicable to actual reactors.

After I looked at it, then I have doubts. I mean, you are convinced that the grid spacer has a first order effect on the rod bundle heat transfer downstream, because it dictates drop size distribution.

DR. HOCHREITER: And turbulent mixing.

MR. SCHROCK: Okay, and whatever else. But when I look through this thing, there is very little of the cross section that is obstructed by those mixing veins, very little.

DR. HOCHREITER: Do you know what is misleading, the rods aren't in here.

MR. SCHROCK: I know, but the mixers don't go all around.

DR. HOCHREITER: No, they don't.

MR. SCHROCK: So you've got portion of the circumference has an area that is -- and droplets that hit that are entrained, and then re-entrained.

DR. HOCHREITER: Or shattered.

MR. SCHROCK: And then the rest of it --

were shattered? I don't know.

DR. HOCHREITER: Were shattered.

MR. SCHROCK: Well, they are hitting at an angle, and I don't know what the velocity is, they are pretty small drops to shatter, I think.

DR. HOCHREITER: Well, let me just -- you may be correct, but that is what we have to find out.

MR. SCHROCK: Well, but what I'm concerned with is will it then eventually be necessary for every vendor to do detailed tests on his grid spacer to establish correlations, or models, or something, that are in the vendor's code, to deal with this part of the reflood heat transfer?

DR. KRESS: I was under the impression you could probably do it just with the area change.

DR. HOCHREITER: Well, that is the first approach. But your point is very well taken, okay? And the real question, I think, that NRR or Research would be asking a vendor is show me why your grid, or the performance of your grid is captured by what we have tested. If you cannot show that, for whatever reason you cannot show that, then you go run a test.

MR. SCHROCK: But in giving them a requirement they might --

DR. HOCHREITER: Only for --

MR. SCHROCK: -- feel on insecure grounds, unless they had hard evidence that, yes, indeed the geometry of the grid spacer is important, and has first order of influence on the results.

DR. HOCHREITER: They know that that is the case. The vendors know that that is the case. That is why you do DNB testing.

The power capability of the fuel assembly, these days, is tied up in the design of the spacers.

CHAIRMAN WALLIS: Does that mean that every vendor has to duplicate your tests with their own spacers?

DR. HOCHREITER: If they want more margin than what we would show.

CHAIRMAN WALLIS: Or less. I mean, how would we know whether they get more or less?

DR. HOCHREITER: Well, they have to -- I would think that you would require them to make an argument that whatever grid they put in is bounded by whatever is tested.

CHAIRMAN WALLIS: Then it is much better to have a test than an argument for something as complicated as that grid.

DR. HOCHREITER: Sure, we can run more tests.

CHAIRMAN WALLIS: Maybe you set yourself up to do a lot of tests now.

DR. HOCHREITER: Well, that was one of the things that we talked about in the program, when we first established the program. Because one of the things that you could is you could test to extremes.

This could represent one extreme, and simple grids, like what you have in FLECHT, would represent the other extreme. And if you can develop a model that will predict both sets of those tests, most of the grids are going to fall, should fall in between, or be closer to this.

DR. SANJOY: What effect did FLECHT show?

DR. HOCHREITER: Very little, if any. And I will show you a plot of that. Again, that is one of the things, one of the new things that came out of this program. now, we weren't looking for it.

CHAIRMAN WALLIS: Now, I think it would be good -- have you finished your description of this? Then we will have a break, and then you can give us results after the break. Would that be appropriate?

DR. HOCHREITER: That would be fine. One of the other pieces of instrumentation we have is this laser illuminated digital camera system, which we photograph through a scattering sheet, through the windows, into the subchannel.

Now, the -- we've calibrated this thing, actually, on a milling machine, so we know exactly what the focal region is. But you are shooting between the rods.

CHAIRMAN WALLIS: So you only have a very short depth of focus in that? The rest of the droplets are out of focus, is that what it is?

DR. HOCHREITER: The other droplets are out of focus because you focus it into the center of the bundle.

MR. SCHROCK: In the shadow, or out of focus?

DR. HOCHREITER: Both, actually. Some of them are shadow, some of them are out of focus. And there is a software package that comes with this system. And you describe in the software package the boundary that you are looking at.

What we don't see, and what we exclude from our sampling, are drops which are hidden by the rods. So you would not count this drop, you would not count this drop, you won't even see this drop. You will count these drops.

CHAIRMAN WALLIS: But it takes a very small mass fraction of drops, or let's say, void fraction, liquid fraction before you just have a fog, and there is no direct line of sight between the laser and the camera, at all.

DR. HOCHREITER: You don't have that many drops.

CHAIRMAN WALLIS: You must have very, very few drops, then?

DR. HOCHREITER: You do.

CHAIRMAN WALLIS: Then there is no way you are going to measure your liquid fraction very accurately with delta P cells.

DR. HOCHREITER: I said that. Yes, you won't. Delta P cells are going to be most accurate at the quench front.

CHAIRMAN WALLIS: Okay. So you are interested in those few drops that make it way ahead of all the others, and may do some cooling way downstream?

DR. HOCHREITER: Well, you know, let me show you the stuff first.

CHAIRMAN WALLIS: Okay.

DR. HOCHREITER: We can talk about the matrix for a minute. We did run tests over this range of conditions. This was not successful, this overheated, and we had to terminate the test.

But the pressure -- we primarily were concentrating on flooding rates around one inch a second, plus a whole series of tests at six inches a second, a few tests at eight inches a second.

The six inches a second test were basically to look at inverted annulus from boiling. This is where the delta P cells would be the most accurate, because you have the most mass in the bundle.

And then look at dispersed flow film boiling, where the flooding rates are one inch a second. We looked over this pressure range, a wide range of subcoolings.

Our temperatures, our initial temperatures, most of the tests were run at 1,400 degrees fahrenheit, and the power, most of the tests were run with .4 kilowatts per foot, and the power was held constant.

And, again, this was to, basically, stretch the transient out in time, and give you more of a quasi state of --

MR. SCHROCK: That seems very low. Earlier you were talking about eight kilowatts per foot.

DR. HOCHREITER: That is the total power

of the rod.

CHAIRMAN WALLIS: Why don't you use international units?

DR. HOCHREITER: Because this is America.

DR. BAJOREK: Larry, I think you said originally that the initial steady state power is 15/16 kilowatt per foot.

DR. HOCHREITER: For the plant, that was for the plant.

DR. BAJOREK: For the plant. What this is, this is at decay power.

DR. HOCHREITER: Okay, thank you. I've already kind of said this already, that the grids have a significant effect. Maybe before we get into the data we could look at this film.

CHAIRMAN WALLIS: Well, you said this is America, but most students, our students are all told international units. They get very irritated when they see things like inches and they don't know what to do with them.

DR. HOCHREITER: Yes, I know, I'm teaching an undergraduate course in reactor engineering, and I make them use english units, they hate it.

CHAIRMAN WALLIS: These are in American thermal units, are they?

DR. HOCHREITER: Well, they are British. This is a spacer grid.

CHAIRMAN WALLIS: This is a movie, now?

DR. HOCHREITER: This is a movie.

CHAIRMAN WALLIS: Maybe we need the lights down.

DR. HOCHREITER: This is being heated up adiabatically, so this is red, this is red, and this is pretty poor.

CHAIRMAN WALLIS: It is going to get redder, is it?

DR. HOCHREITER: It is pretty red right now.

CHAIRMAN WALLIS: Why are they so wiggly, those rods?

DR. HOCHREITER: Why are they so wiggly? I think it is more because the camera is --

CHAIRMAN WALLIS: They don't look straight, they've got bulges, and wiggles, and --

DR. HOCHREITER: I think the camera is at an angle here.

CHAIRMAN WALLIS: Is it the heat flux, is it some thermal boundary layer distortion?

DR. HOCHREITER: No, I think it is a camera. What you are seeing is reflood has started.

This is still hot below the grid, this is dark. So there is good cooling downstream on the grids.

CHAIRMAN WALLIS: It is not obvious to me.

DR. RANSOM: Is that just steam going through there?

DR. HOCHREITER: Steam, and it is hard to see with this film, but drops are going through.

CHAIRMAN WALLIS: The rods look bigger downstream than upstream. And they don't --

DR. HOCHREITER: That was really tricky to me.

CHAIRMAN WALLIS: They don't look continuous.

DR. HOCHREITER: Well, they are.

CHAIRMAN WALLIS: They have a jog in them. Those are the rods, those things, those shiny things are the rods downstream?

DR. HOCHREITER: Yes, these are the -- the shiny hot things.

CHAIRMAN WALLIS: Why is it so dark in between them?

DR. HOCHREITER: This is the spacer grid.

CHAIRMAN WALLIS: No, no, between the shiny rods downstream.

DR. HOCHREITER: Why is it so dark in

here? Because it is cooler. If you come up here you can see drops zipping past her.

MR. SCHROCK: Yes, I can see that from here, even. But what is the background there, what is hot behind the lower portion?

DR. HOCHREITER: More rods.

MR. SCHROCK: Now, is there a metal back behind there that is glowing red?

DR. HOCHREITER: I don't quite understand.

MR. SCHROCK: Well, the back wall of the channel is what?

DR. HOCHREITER: Well, the back wall of the channel is going to be seven rows away.

DR. RANSOM: So you are looking at an angle.

MR. SCHROCK: In spite of that, you are looking at a clear shot through there, and it --

DR. HOCHREITER: No, it is not a clear shot, because you are looking at somewhat of an angle, because otherwise you would see all the way through these rods.

MR. SCHROCK: You are not seeing all the way through.

CHAIRMAN WALLIS: So there is a whole host of droplets in between those rods down below?

DR. HOCHREITER: There are drops and steam that are coming up through here.

CHAIRMAN WALLIS: Then they all go dark downstream.

DR. HOCHREITER: So you can see much more of the effect up here.

CHAIRMAN WALLIS: Why do they go dark downstream?

DR. HOCHREITER: Because of the cooling.

DR. RANSOM: Does that look like the entrainment on that spacer grid? I can see something fluctuating off of it.

DR. HOCHREITER: Right here?

DR. RANSOM: Yes.

DR. HOCHREITER: Probably.

CHAIRMAN WALLIS: What does cooling have to do with the color of the droplets?

DR. HOCHREITER: It is not the droplets, it is the rods. Graham, these are rods.

CHAIRMAN WALLIS: Those shiny things are rods?

DR. HOCHREITER: Yes.

CHAIRMAN WALLIS: But the dark spaces in between are droplets.

DR. HOCHREITER: No, it could be more rods behind here, which are also dark and cooled.

CHAIRMAN WALLIS: Oh, I thought you were looking right through the rods.

DR. HOCHREITER: No. This is at a slight angle.

MR. SCHROCK: In the bottom picture you can almost see the outline of the edge of the next row of rods?

DR. HOCHREITER: Right. Bottom line is, hot, cold.

CHAIRMAN WALLIS: I guess the thermacouples show that?

DR. HOCHREITER: Yes.

CHAIRMAN WALLIS: Now, there seems to be some pulsations going on.

DR. HOCHREITER: It is not perfectly steady.

CHAIRMAN WALLIS: It looks like a lot of pulsations now have developed.

DR. SANJOY: The quench front is approaching?

DR. HOCHREITER: Quench front is about ten feet away.

CHAIRMAN WALLIS: The quench front is down below somewhere?

DR. HOCHREITER: Yes.

CHAIRMAN WALLIS: Are we going to see it?

DR. HOCHREITER: I don't think.

DR. SANJOY: You can see it, but it takes a long time.

DR. HOCHREITER: If we want to go off for coffee and come back.

CHAIRMAN WALLIS: Now, do those rods go red again before the next spacer grid?

DR. HOCHREITER: I actually don't really know because the temperature goes back, so I think it does.

DR. KRESS: They do in the Oakridge test, they get hot again at the top.

CHAIRMAN WALLIS: Now, what are those shiny white bubbly things that are above the grid?

DR. HOCHREITER: These are probably the veins, and you are probably seeing the liquid.

CHAIRMAN WALLIS: Those are veins.

DR. HOCHREITER: I would say the velocity in the bundle is some place between and 60 feet a second.

DR. MOODY: You are injecting a spray?

DR. HOCHREITER: No, this is a reflood of inch a second.

DR. MOODY: Because of the quench, all right, that is sending the droplets up in the vapor, then.

DR. HOCHREITER: Yes, it is easier to see them here than it is here.

MR. SCHROCK: How far is the quench front below this?

DR. HOCHREITER: I can't answer that, I would have to go back and --

MR. SCHROCK: Is it a long way, or short?

DR. HOCHREITER: I think so, yes.

MR. SCHROCK: Long ways.

DR. HOCHREITER: Although you are starting to see this cool down now.

CHAIRMAN WALLIS: Why didn't that get cooled before? Because when the quench front was below that, it was above a spacer.

DR. HOCHREITER: I'm sorry?

CHAIRMAN WALLIS: I mean, it just seems funny that you have so much cooled so well up above.

DR. HOCHREITER: That is because the spacer grid is mixing up this --

CHAIRMAN WALLIS: Why isn't --

DR. HOCHREITER: -- shattering drops --

CHAIRMAN WALLIS: -- the red part cooled

by the spacer which is below it?

DR. HOCHREITER: Because there is another two feet before the next spacer.

CHAIRMAN WALLIS: Oh, it is a long way.

DR. HOCHREITER: There is like 40 to 45 --

DR. SANJOY: Where is the --

DR. HOCHREITER: -- spacers.

DR. SANJOY: -- flux peak?

DR. HOCHREITER: Ralph, do you remember which elevation this was?

DR. ROSAL: It is 105, that grid is 110.

DR. HOCHREITER: So the flux peaks right about here.

DR. ROSAL: Elevation for the power, it is below the grid.

DR. HOCHREITER: Now everything is starting to get cool. So the quench front is moving up.

MR. SCHROCK: Now, why do they look different in the two zones?

DR. HOCHREITER: Why do they look different?

MR. SCHROCK: Why are the rods shiny on top and not --

DR. ROSAL: Because of the light that is

shining on the window.

CHAIRMAN WALLIS: Okay, so you can change what you see by how you illuminate it.

MR. SCHROCK: Are they glowing red, is that --

DR. HOCHREITER: At the beginning of the test they were glowing red.

MR. SCHROCK: When the test is first initiated, and that top zone is all glowing red, then if you can view the whole length of it, do you see the precursory cooling affecting the lower part of it first, and then propagating up into the upper part of it?

DR. HOCHREITER: It is really not affecting this very much at all.

MR. SCHROCK: No, I'm talking about this upper zone now. What you showed us, you began with it already cool there. But if I could see the top of that?

DR. HOCHREITER: It would probably be red.

MR. SCHROCK: Still glowing red?

DR. HOCHREITER: Yes.

MR. SCHROCK: So your model is going to have to take that kind of thing into account.

CHAIRMAN WALLIS: That thing which is up, there is a light just above your head there?

DR. HOCHREITER: You are starting to -- this is starting to get liquid on the --

CHAIRMAN WALLIS: The thing above your head, there, is a light, that is why it is white above?

DR. HOCHREITER: You mean right in here? See, you are starting to get liquid up, now. A lot of liquid.

CHAIRMAN WALLIS: Something is bouncing up and down.

DR. HOCHREITER: We may have already quenched this down here. It is hard to see where the quench front is.

DR. MOODY: That is real time?

DR. HOCHREITER: Actually I think that this is faster than real time.

DR. SANJOY: That is oscillatory behavior.

CHAIRMAN WALLIS: Yes, it does look oscillatory.

DR. HOCHREITER: This is typical behavior for a reflood test.

CHAIRMAN WALLIS: It doesn't look very analyzable to me.

DR. HOCHREITER: Well, it is time average, your calculation is going to be averaging this over time. It might be six months, a year.

But the -- if you induce system oscillations they are much more pronounced. I mean, they are huge oscillations that go up through the entire bundle.

CHAIRMAN WALLIS: There are, even in your tests?

DR. HOCHREITER: When we had poor pressure control, yes.

CHAIRMAN WALLIS: But not in the Penn State tests?

DR. HOCHREITER: No, in the Penn State tests when we had poor pressure control you could get large surges in the oscillations, and you would see it on the data. You would see it in the thermacouples --

CHAIRMAN WALLIS: Which happens in the reactor, do you get these large surges, or not?

DR. HOCHREITER: Again, according to Long Sung Tong, reactors don't oscillate.

MR. SCHROCK: Larry why is it heating up above the spacer grid? The spacer grid at that point in the transient doesn't seem to be effective in inducing any cooling.

DR. HOCHREITER: Right now, you mean?

MR. SCHROCK: Yes, it is hotter up there than it is down below.

DR. HOCHREITER: I think you are seeing the shine.

DR. BAJOREK: Larry, right above, if you raise your left hand, higher, higher, over to that, right there, that is the light, that is the trouble light.

MR. SCHROCK: Well, do they keep moving the light around?

DR. BAJOREK: No, it was the same place.

MR. SCHROCK: It was bright down below when you started out.

DR. BAJOREK: That was the rods. It looked like the electric burners on a stove.

DR. HOCHREITER: Why don't we stop and back this, rewind this thing? There is a combination of things going on, light and --

CHAIRMAN WALLIS: We will take a break after this movie, if that is okay with you.

MR. SCHROCK: They really did look like they were distorted when they were red hot. What do you do to prevent axial compression when they heat?

DR. HOCHREITER: Axial compression?

DR. KRESS: They are only tied at one end, I think.

DR. HOCHREITER: They are supported at the top, and they go through the bottom. The rods are supported at the top, they screw into a top plate. And there are o-ring seals at the bottom, so the rods grow downward in a thermal expansion.

MR. SCHROCK: But they connect to some rigid piping somewhere, so --

DR. HOCHREITER: They go into a molten pool, which provides, basically, the ground to return current pool.

MR. SCHROCK: SO they just hang there?

DR. HOCHREITER: Yes.

DR. SANJOY: What is the molten metal?

DR. HOCHREITER: Lead.

CHAIRMAN WALLIS: This is how you get the electrical contact?

DR. HOCHREITER: Take it almost right back to the beginning.

CHAIRMAN WALLIS: We can watch it backwards.

DR. MOODY: Larry, that far left strip, is that the other side of the --

(Everyone speaks at the same time.)

DR. MOODY: It looks like a window on the very left strip, there.

DR. HOCHREITER: Here?

DR. MOODY: No, now move over -- yes, up and down there.

DR. HOCHREITER: I think this would just be the aluminum insulation, the aluminum coating around the --

DR. MOODY: When we saw that activity in the other, water and so forth, you could see something in that section too. I just wondered --

DR. HOCHREITER: Here?

DR. MOODY: Yes.

DR. HOCHREITER: I hope not. That means the window was leaking.

DR. MOODY: Well, it looked like looking through a window. Is it angled such that we are seeing some of the same activity in that strip?

DR. HOCHREITER: Well, I hope not. Well, if the window leaks, this is hot --

CHAIRMAN WALLIS: So you want to run it again, or something? What do you want to do, Larry?

DR. HOCHREITER: That is your choice.

CHAIRMAN WALLIS: Are we going to see anything different the second time?

DR. HOCHREITER: I think you will, yes.

CHAIRMAN WALLIS: Well, what I suggest we do is we take a break, and you run the movie during the break, and if anybody wants to see it again, they can see it. Then we can have an informal discussion, off the record, during the break if that helps.

We will break for 15 minutes, come back at 3:25, and we will run the movie during the break for those who want to see it again. But we will break now.

(Whereupon, the above-entitled matter went off the record at 3:11 p.m. and went back on the record at 3:25 p.m.)

CHAIRMAN WALLIS: Let's come back into session.

We are now going to hear what we've been looking forward to, which is the description of some of the data produced by this wonderful setup.

DR. HOCHREITER: Okay. What I have is data for two tests, and I was going to run through that. And maybe after you see the first test we can go through the second test faster.

The main change is, primarily, the pressure. So this is a 20 PSI experiment, one inch a second flooding rate, 1,4000 degree initial temperature, 20 degrees subcooling.

The first thing I would point out is the duration of the test. It is really very long. And the reason for this is using constant power. And you tend to, basically, be able to get longer periods of time of dispersed flow of film boiling.

And this is the quench front from the heater rods as the quench basically moves up. So this is basically quench elevation versus time.

DR. KRESS: That doesn't look like one inch per second.

DR. HOCHREITER: Why doesn't it look like one inch a second?

DR. KRESS: Well, between 400 and 600 seconds, that is 200 seconds, and it would be 200 inches change, and I don't see 200.

CHAIRMAN WALLIS: I think is --

DR. HOCHREITER: No, one inch a second is the cold flooding rate.

CHAIRMAN WALLIS: It is the flow rate of water.

DR. HOCHREITER: Cold flooding rate.

DR. KRESS: I see what you mean. You are losing -- that stuff with steam.

DR. HOCHREITER: About 95 percent of it.

DR. KRESS: Yes, I see.

CHAIRMAN WALLIS: How do you define quench front?

DR. HOCHREITER: Let me show you in a figure for a minute. Can I hold that for a second?

The next slide is a calculation for one parameter, and then the other is data. This is where we calculate the saturation line to be, the bundle, this is where the energy comes.

And, again, the power is constant, the flow is constant, subcooling is constant in the test. So basically you would start to boil at this point. And the red line is basically the rod quench front, the black line is the housing quench front. These are data.

So this region between the red line and the blue line, basically is a two phase region, where you basically have nuclear boiling. And so you have production of steam in this region, in addition to the steam that is generated when you quench the rods from the stored energy release of the rods themselves.

CHAIRMAN WALLIS: You have steam created above the quench front too.

DR. HOCHREITER: You have steam created above the quench front due to evaporation of the droplets.

This is something that is very different than other reflood experiments, for two reasons. One, most of the tests were run with higher subcoolings, typically 150 degrees subcooling.

Two, the power was not constant. So if you would plot the saturation line for those experiments it basically followed the quench front, and then peeled it away from the quench front at about this time period here.

So, again, by running with a constant power you basically have expanded the region boiling below the quench front, but then you've expanded the time duration of the test.

DR. KRESS: So if I look below that blue line?

DR. HOCHREITER: It is single phase liquid.

DR. KRESS: -- would be one inch per second, then?

DR. HOCHREITER: Below this, yes.

DR. KRESS: It doesn't look like it.

DR. HOCHREITER: Well, probably because I have only drawn it to here.

DR. KRESS: Okay.

DR. RANSOM: What do you mean by sat line?

That position is where you exceed the saturation temperature, is that right?

DR. HOCHREITER: Of the coolant, yes. Just from an energy balance.

DR. SANJOY: It is a very low reflood rate?

DR. HOCHREITER: Right, low reflood rate, low subcooling.

Now, what I've got are a bunch of plots of temperatures above and below spacer grids at different elevations. This is for the grid that is at the 69 inch elevation, the black thermocouple is thermocouple on the rod, and the inner five by five is located at this elevation.

The green one, which I've colored in, is a thermocouple here. You asked about where the quench front is. Quench front is defined, usually, by the need in this curve. Because that is where we think that you start to get wetting.

CHAIRMAN WALLIS: Where it begins to turn down rapidly?

DR. HOCHREITER: Very rapid, yes. And we've used a criteria to look at this, so many degrees per second for quenching.

Now the thing that is very apparent from this figure is that the cooling, as we saw on the film, downstream, is better than the cooling upstream. And that is really reflected in this temperature difference.

CHAIRMAN WALLIS: Until the quench front gets close?

DR. HOCHREITER: Right. And then there is a difference here, because there is an elevation difference between the two T cells.

I've got a series of these plots, and I also have vapor temperature measurements. These are vapor temperature measurements, again, upstream and downstream of the quench front.

This steam probe is up here, it is black -- I'm sorry. Can you hear me?

So we have a steam probe here, and a steam probe here. This one is at 83 inches above the grid, this one is below the grid at 16 inches. You don't see a lot of difference in through here, but you see, again, a continuation of the vapor superheat, really out for a pretty long period of time.

Now, these --

DR. SANJOY: These are the same run that you --

DR. HOCHREITER: Same run. The way, at

least, I'm interpreting the steam probe measurements, and this is open for debate, I probably shouldn't have said that, is that the real steam temperature is the peaks of this.

This is really the thermocouple seat water droplets. What is unique about these measurements is that you see a persistence of superheated vapor for a long time.

In the previous reflood experiments that I have looked at, in tests which I have run myself, you would not get vapor superheats that would be persistent for as long a period of time, and at an elevated superheated temperature.

CHAIRMAN WALLIS: What is interesting to predict is not just the peak, but what seems to be the lower, which is around 200 degrees C, in your green curve, there is a whole range of --

DR. HOCHREITER: Well, or the difference.

CHAIRMAN WALLIS: Why is it bottoming out at 200?

DR. HOCHREITER: In here?

CHAIRMAN WALLIS: Yes.

DR. HOCHREITER: I don't know, I don't have a good answer for that. This is a saturation temperature, essentially, here. The 20 PSI test, saturation is 228 degrees fahrenheit.

CHAIRMAN WALLIS: Well, maybe that is the radiation from the rods.

DR. HOCHREITER: That is keeping it up here?

CHAIRMAN WALLIS: Right. Because quench front comes by around 500 or something?

DR. HOCHREITER: Well, that is what I was going to show next. This is at 73 inches.

CHAIRMAN WALLIS: Well, it is not the same scale of time.

DR. HOCHREITER: Well, I know, so that is here. It is about 700 seconds, this thing is quenching, going to saturation at about 600 seconds.

CHAIRMAN WALLIS: But the previous slide is much better, yes. So the thermocouple quenches before the rods do, before the clad does?

DR. HOCHREITER: The steam probes tend to quench before the rods do, yes. This is quenching at about 600 seconds. Actually, this is pretty close.

CHAIRMAN WALLIS: Your scales aren't the same, are they?

DR. HOCHREITER: No, but this time and this time are the same. But, again, I have never run experiment, or seen tests where the vapor remains superheated for a longer period of time, as the rods start to quench.

CHAIRMAN WALLIS: So that is something that your theory is going to explain, or model?

DR. HOCHREITER: Well, somebody's model, yes.

CHAIRMAN WALLIS: Not yours?

DR. HOCHREITER: I don't know, maybe. So, I mean, to me this is great stuff for any kind of an advance code, because you have to try to predict this. Now, again, I think you have to use some constructive interpretation of the measurement.

And it is really the peaks that I think you want to look at. And then it really gets fuzzy in here.

DR. SANJOY: What is a bit surprising is that the green and the black back there --

DR. HOCHREITER: Are about the same?

DR. SANJOY: Yes.

DR. HOCHREITER: I know. You don't see that in all of them. Let me run through some more. This is low, this is fairly low in the bundle.

CHAIRMAN WALLIS: Maybe it is only steam up there, there is no water at all.

DR. HOCHREITER: No, there is water. If

there is only steam you would not see this.

CHAIRMAN WALLIS: You wouldn't?

DR. HOCHREITER: No.

MR. SCHROCK: What is the meaning of SP and CT, that is on your location up there?

DR. HOCHREITER: CT is clad thermocouple, ST is steam probe. So these are clad thermacouples inside the heater rods, these are steam probes.

MR. SCHROCK: They are all temperatures?

DR. HOCHREITER: Yes, yes. I'm going to get this all in the border. Now, this is another elevation that is further up.

CHAIRMAN WALLIS: This is now in centimeters a second?

DR. HOCHREITER: I have a foreign student doing this. And by contract we have to give the NRC this stuff in metric. The only problem is I don't understand it. I do understand that.

CHAIRMAN WALLIS: Well, centimeter is not a standard unit.

DR. HOCHREITER: So this is at, with the grid at the 89 inch elevation. Again, this is a clad temperature at 91 inches, and a clad temperature at 88 inches.

And you can see the effect of the spacer grid is to drop this by over 200 degrees C.

CHAIRMAN WALLIS: Now, all these curves are digitized, and recorded in electronic forms?

DR. HOCHREITER: Yes.

MR. SCHROCK: Where is the grain? Is that the upper thermocouple on the plan?

DR. HOCHREITER: Yes. 91 inches.

DR. KRESS: Now, have you replotted these anywhere as the temperature versus elevation?

DR. HOCHREITER: Yes.

DR. KRESS: Above the --

DR. HOCHREITER: Yes.

DR. SANJOY: But the first peak is about the same, right?

DR. HOCHREITER: Yes, and this is probably because this is right at the beginning of the test, and there probably is no water.

CHAIRMAN WALLIS: So how does the steam get heated so much as it goes through the grid?

DR. HOCHREITER: The steam is getting cooled.

CHAIRMAN WALLIS: No, the next curve. The steam probe is the next one. In this one the steam is getting heated as it goes through the grid, isn't it? Am I looking at something else?